

SIR JOHN HERSCHEL AND THE STABILITY OF SATURN'S RING

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In a pioneering exposition of mathematical astronomy for the public, Sir John Herschel attributed the stability of the ring of Saturn to its being eccentric with respect to the planet and lopsided (asymmetric in mass) by a minute amount. Tracing the sources and effects of this error reveals several lessons of general relevance to science: on the formulation and interpretation of calculations, the use of cutting-edge observations and the combining of observations with theory. I emphasise the phenomenon of reinforcing errors.

Astronomy for the Public in 1833

Sir John Herschel (1792-1871) was not only a major figure in astronomy and related sciences in the middle of the nineteenth century, he was a pioneer in explaining the workings of a highly mathematical subject to the general public. His *Treatise on Astronomy*¹ of 1833 wrestled with the difficulty of describing the workings and results of mathematical physics (including the abstruse theory of the mutual perturbations of the planets) without actually displaying the equations. He must have been at least moderately successful, since the *Treatise*, in its somewhat rewritten form as *Outlines of Astronomy*², went to eleven editions, with the latest coming out in 1869 (and being reprinted at least as late as 1902).

Perhaps surprisingly, almost all of his 1833 book could be used in a university course in astronomy today. The knowledge set out in it has stood the test of time (especially the details of the planetary orbits) and Sir John was careful in warning his readers about what was speculation and what was simply unknown. (Of course, a book written before the discovery of Neptune, the use of spectroscopy and the formulation of thermodynamics has its limitations for later use, a point made as early as the 1902 reprinting).

However, for this study I am going to examine one of the mistakes, one of the few statements made by Herschel without warnings or caveats that turned out to be wrong. The episode illuminates the workings of our science, some details of its progress and a bit of the philosophy of its practitioners.

Herschel on Saturn's Ring

After introducing Saturn, describing the appearance of the planet and giving various numbers (dimensions, distance and such), Herschel¹ says

Although the rings are, as we have said, very nearly concentric with the body of Saturn, yet recent micrometrical measurements of extreme delicacy have demonstrated that the coincidence is not mathematically exact, but that the center of gravity of the rings oscillates round that of the body describing a very minute orbit, probably under laws of much complexity. Trifling as this remark may appear, it is of the utmost importance to the stability of the system of the rings. Supposing them mathematically perfect in their circular form, and exactly concentric with the planet, it is demonstrable that they would form (in spite of their centrifugal force) a system in a state of *unstable equilibrium*, which the slightest external power would subvert—not by causing a rupture in the substance of the rings—but by precipitating them, *unbroken*, on the surface of the planet. (§444, p. 284; here, as elsewhere, I retain the emphasis of the original publication)

In the same section but on the next page he goes on to say that, to ensure stability,

...it has been shown this it is sufficient to admit the rings to be *loaded* in some part of their circumference, either by some minute inequality of thickness, or by some portions being denser than others. Such a load would give to the whole ring to which it was attached somewhat of the character of a heavy and sluggish satellite ...

There are a number of things to take issue with in this section and neighboring ones. Among them, Herschel later confusingly insists that ‘we have no proof’ of the ring being loaded, and offers some comments on stability that are more difficult to interpret. For our purposes, however, there are four important points: the assumption that the ring is a solid body; the assertion that a uniform ring would be unstable; that the smallest amount of non-uniform mass distribution would impart stability; and that an eccentricity, a displacement of the rings from perfect concentricity with the planet, had been observed. The idea of the rings being solid would lead to a lengthy historical investigation that I will not attempt here; I note it simply as a background to the issue of stability. The second two assertions belong to dynamical theory and, of course, could not be expounded in a work designed without equations; we will examine their source in the next section. I will look at the observational story at in the section following.

The theory of rings

Not only is a popular work without mathematical derivations, it generally lacks the footnotes that would direct one quickly and easily to their origin; so it is in this case. A secondary source, Alexander³ describes a stability calculation which sounds much like Herschel’s assertions, attributing it to a memoir by Laplace⁴. However, a reading of that memoir (very helpfully published on line by the Bibliothèque National de France) reveals nothing of the kind. Alexander

appears to be paraphrasing a description by Proctor (no specific reference is given), so perhaps he mixed up his references or Proctor did; in either case we appear to have another instance of the Law of Propagation of Bad Data.

However, both Herschel's assertions and Alexander's description fit very well a section of Laplace's *Mécanique Céleste*⁵. Conveniently for many of us it was translated into English and provided with an extensive commentary (roughly doubling its size) by Nathaniel Bowditch, an American mathematician and navigator, his first volume appearing in 1829. This is the version I have consulted, though (considering publication dates and the slowness of trans-Atlantic travel at the time) Herschel more probably referred to the original French publication of 1799.

In Book III, Chapter VI, §46, Laplace considers the stability of a uniform solid hoop under the gravitational pull of a planet. If the hoop is centred on the planet it is in equilibrium, since each part of the hoop is subject to a force that is exactly counteracted by a part on the opposite side of the planet. If the hoop is disturbed, what happens?

Setting up the integral for gravitational potential to decide the matter, Laplace found no simple form but succeeded in transforming it into a series that is a monotonically decreasing (increasingly negative) function of the displacement of the hoop from concentricity. In his words,

... therefore the centre of Saturn repels the centre of this circular homogeneous ring; and whatever be the relative motion of the second centre about the first, the curve it describes, by this motion, is convex towards Saturn; the centre of this circular ring must therefore recede more and more from the centre of the planet, until its circumference shall finally come in contact with the surface of the planet.

...

Hence it follows, that the separate rings which surround the body of Saturn, are irregular solids, of unequal widths in the different parts of their circumferences; so that their centres of gravity do not coincide with their centres of figure. These centres of gravity may be considered as so many satellites, which move about the centre of Saturn, at distances depending on the inequalities of the parts of each ring, and with velocities of rotation equal to those of their respective rings. (pp. 515-516)

In his extensive footnote Bowditch explains the second paragraph by showing that *for a geometrically centred ring* (a tacit, but important, assumption) the centre of gravity of the hoop is moved off the geometric centre by its mass loading, and that the centre of gravity is now pulled toward the centre of the planet rather than being expelled.

I will take each assertion in turn. First, there is one major flaw in Laplace's proof of the instability of a symmetrical ring: the failure to take motion into account. Although the rotation of the rings was clearly known to him (and indeed used in calculations in the previous section) it is not included in the

mathematics of stability. Perhaps the fact that the planet can exert no net torque on a symmetric ring led him to dismiss it as a factor. But even in a symmetric case, as a spinning top or a figure skater, the rotation does have an effect on the dynamics through possible changes in the moment of inertia. A satellite, indeed, would fall directly into the planet without its motion of revolution.

The mention of a satellite brings up another problem: the confusion of static with dynamic stability; or, perhaps, the failure to notice that they are different concepts. This I will take up in some detail below.

In the proof of the stability of the loaded ring there are two errors, either of which reduces it to nonsense. The first is that a loaded ring that is geometrically centred on the planet is *not* in equilibrium—obviously, since the forces are not in balance; so one *cannot* take this position as the beginning of a stability analysis. Second, the motion of the centre of mass is quite secondary to that of the ring itself. The loaded ring is still drawn towards the planet along one part of its circumference; and Laplace is silent on what might happen after the centres of gravity of the loaded ring and the planet coincide. We are left, at the very best, with a feeling that a loaded ring might behave something like a satellite of the planet, and a feeling or analogy is not a proof.

So the dynamical basis of Herschel's assertions, the instability of a symmetrical ring and the stability of a loaded one, has been shown to be mistaken. (Note that neither assertion has been proven to be wrong! I will consider both questions in detail below.) All in all, this is a disappointing performance by three very competent mathematicians.

Measuring the rings

An account of the beginning of the observational side of this episode is to be found in the *Philosophical Magazine* for 1828 July⁶. On pages 62-3 are found summaries of two letters. First, one from Professor Harding of Göttingen related how Heinrich Schwabe had gained the impression that the eastern side of the rings of Saturn were farther from the planet than the western side, and the two had agreed over several months (1827 December to 1828 May) that it appeared to be so. Harding thought it was an 'optical deception' but could not explain it, and asked astronomers with better telescopes and more precise instruments to look into the matter.

The response of James South to this request appears immediately afterward in the same issue. South and several other observers, including Sir John Herschel, observed the planet on 1828 April 26, 29 and May 8 using South's 'five-foot' refractor (the lens of which would have been about five inches in diameter). The average of 35 micrometer measurements made by South and Herschel gave the space between the planet and the ring as 3.535" on the west side and 3.607" on the east, a difference South considered insignificant. On April 26 each of the two observers made ten measurements, Herschel getting an average of 3.612" on the west, 3.442" on the east, while South's averages were 3.331" on the west and 3.502" on the east. Still, Herschel thought the gap

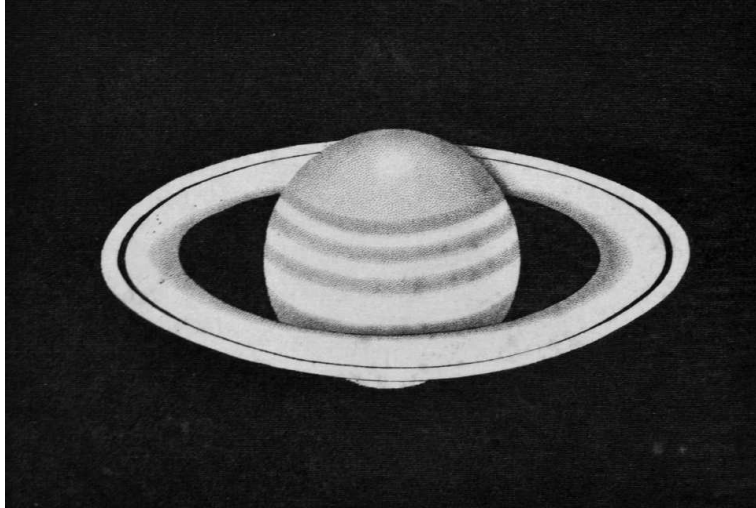


Figure 1: An engraving of Saturn from Sir John Herschel’s *Treatise on Astronomy* from 1833². The eccentricity as measured by F. G. W. Struve amounts to slightly less than half a millimetre on this figure as printed.

between the ring and the planet appeared larger on the east side, and six of the seven observers present agreed. However, South writes that Saturn was low in the sky and the observations were ‘far from satisfactory.’

The challenge was next taken up by F. G. W. Struve, the consummate observer of double stars and perhaps the best person in the world at the time at measuring tiny features on the sky. His report appears in 1828 May⁷. Using the 9.5” Fraunhofer refractor at Dorpat (now Tartu) he observed Saturn on several nights in 1828 March and April. His results, given in seconds of arc between the planet and the outer edge of the rings and corrected for the changing distance from the Earth to the planet and the phase angle, are as follows:

Date (1828)	Number of observations	West side	East side	difference
March 29	1	11.272	11.390	+0.118
April 7	2	10.996	11.250	+0.254
April 7	3	11.148	11.260	+0.112
April 9	4	10.931	11.243	+0.312
April 10	2	11.238	11.485	+0.247
April 21	3	11.060	11.238	+0.178
total/avg.	15	11.073	11.288	+0.215

Struve gives the likely error for a single observation of 0.095”, which is consistent with the table, and thus the likely error for the average of fifteen measurements is 0.024.” Before his observations Struve ‘considered the difference . . . to be an optical illusion,’ but afterward thought that his measurements ‘prove

the excentricity of the sphere within its rings in the most conclusive manner.’

Before we consider the observations in detail it is worth asking whether it is at all reasonable that Struve could distinguish such tiny details in the first place. Two-tenths of an arc second is a fraction of the Airy disk of the telescopes used; it is not trivial to see or measure this amount. But based on personal experience I think it is possible. Using the eight-inch Alvan Clark refractor at the U. S. Naval Academy, of the same type as Struve had at his disposal though slightly smaller and newer, on a particularly good night I noticed (without looking for it) that the Galilean moons of Jupiter were all of different sizes. This means I was distinguishing between four disks of 0.8” to 1.4” diameter, differences of one or two tenths of an arc second, just what we are considering. The observations are of a different kind and I made no measurements, but I am still led to conclude that Struve’s report is *a priori* plausible.

There are, however, difficulties when we consider the efforts of all the observers. If the eccentricity was so clear to most observers in the party of Herschel and South, why couldn’t they measure it? Indeed, Herschel’s own figures on 26 April went the wrong way. And if the conditions were so poor that measurements of one or two tenths of an arc second could not be made reliably, it strongly suggests that whatever was giving the appearance of eccentricity was not an actual displacement of the rings of this amount.

A hint that Struve’s observations in themselves were not quite what they were taken to be could be gained from looking hard at the table he gave. If we consider each line to be a separate observation instead of an average of a few (which is not quite the correct way to look at them, but will serve to prove this point) we find the standard deviation of the western measurements to be 0.128 and of the eastern 0.097. If they were statistically independent we would expect the standard deviation of their difference to be these added in quadrature, 0.161; instead we have half that, 0.081—indeed, *less* than that of either measurement alone. There is thus some reason to suspect that the standard statistical methods as used by Struve may not be applicable to this set of figures; at the very least a careful review from the beginning needs to be made.

Thus there were reasons to be suspicious of the reported observation of eccentricity in Saturn’s rings. There is, however, a rather stronger reason to question them, one that does not seem to have been considered at the time: a dynamical one. All astronomers involved were well aware that Saturn’s ring revolves quickly around the planet, taking about ten and a half hours for a revolution. Why, then, should the eccentricity, especially if tied to a particular point on the ring (where the mass-load is located), stay on the same side? It apparently did so not only over days but over months and on both sides of opposition. While the others may have been only pursuing a puzzling observation on the edge of current capabilities, Herschel explicitly put observation and theory together and should indeed have noticed the dynamical problem.

Afterward: Maxwell and another Struve

Herschel's eccentric Saturn seems to have had little effect on the progress of astronomy. Although he retains essentially the same passage in his later popular astronomy books (even in the edition of 1869), I have found few other references to the matter.

As far as observation goes, the elder Struve's result of 1828 was not confirmed. His son Otto Struve, using the much larger refractor at Pulkovo in 1851, found no significant eccentricity^{8*}. What caused the strong impression of eccentricity, and what the elder Struve actually measured, remain unknown.

The dynamical theory of Saturn's rings received more attention. What came to be considered the definitive answer was set out by James Clerk Maxwell at the outset of his scientific career⁹. The Adams Prize for 1856 set the problem of calculating the dynamics of the ring system of Saturn, allowing the contestants to choose what form they were to assume the rings took. That is, 'It may be supposed that (1) they are rigid; (2) that they are fluid and in part aeriform; (3) that they consist of masses of matter not materially coherent' (p. 286). Maxwell looked at each possibility in turn, but it is only the first that concerns us here.

Maxwell begins by pointing out that a solid object of planetary size, especially one so broad and thin, cannot maintain itself by its own strength 'though it were made of the most rigid material known on Earth' (p. 287), a point also made by both Laplace and Herschel, and thus that it must be in 'dynamical equilibrium' (p. 286), a very important phrase. Considering Laplace's results, Maxwell says that

he proves most distinctly (Liv. iii Chap. vi) that a solid uniform ring cannot possibly revolve around a central body in a permanent manner, for the slightest displacement of the center of the ring would originate a motion that would never be checked, and would inevitably precipitate the ring upon the planet, not necessarily by breaking the ring, but by the inside of the ring falling on the equator of the planet (pp. 293-4)

However,

I have not discovered, either in the works of Laplace or in those of more recent mathematicians, any investigation of the motion of a ring either not uniform or not solid (p. 294).

So Maxwell accepts Laplace's result on instability, to the point of repeating the claim that the ring would be unbroken upon striking the planet's surface. (This is interesting in the context of his emphasis on the role rigidity cannot

*In that publication he did, however, assert that the ring system as a whole was of a different size than earlier measurements allowed, and thus must be changing on the time scale of decades to centuries. To follow this observational tangent would obviously take me too far from Herschel and the matter at hand. Of relevance to our question is Otto Struve's comment that his measurements of the inner edge of the rings are of less precision because of the difficulty of defining the border of the newly-discovered crepe ring

play in the whole situation. It appears that all of our scientists, from Laplace on, have ignored the very serious differential stresses that would act on the ring during its fall onto the planet.) However, Laplace's assertion of the stability of a loaded ring is simply ignored. Perhaps Maxwell thought it merely an analogy or plausibility argument (as indeed it is), rather than a serious attempt at calculation.

In using the phrase 'dynamical equilibrium' above Maxwell has hinted at a deeper and more complex idea of stability than that exhibited by Laplace, Bowditch or Herschel. Before actually starting his work he makes this explicit:

There is a very general and important problem in dynamics, the solution of which would contain all the results of this essay and a great deal more. It is this—

'Having found a particular solution of the equations of motion of any material system, to determine whether a slight disturbance of the motion indicated by the solution would cause a small periodic variation, or a total derangement of the motion.' (pp. 295-6)

This is the criterion of dynamical stability that Maxwell uses, though he does not use the term explicitly here.

Maxwell then addresses Saturn's rings as a collection of thin, rigid hoops, circular but not necessarily uniform in mass around their circumference, and investigates the dynamical stability of one representative example. He writes down the equations of motion, allowing for the displacement of the centre of mass of the ring from that of the planet; rotation of the ring about its own centre of mass; and rotation of the line joining the centres of mass.

He next imposes, as his dynamic equilibrium, a state of uniform rotation. This is interpreted to mean that the 'position of the centre of the planet with respect to the ring does not change' (p. 299) and that the rotation of line joining the centres of mass proceeds at a constant rate. As a condition for this state Maxwell finds that the gravitational potential of the ring must be at a stationary point; in other words, the planet must occupy a (possibly local) minimum or maximum of the hoop's potential field[†]. This means that Maxwell's treatment is not general enough to include, as a limiting case, all the ring's mass concentrated at a point (that is, a small satellite)[‡].

These appear, at first sight, to be a quite restrictive set of conditions. Certainly one could imagine a hoop in some sort of motion in which distances and angular velocities vary within limits; that is, one might allow the analogy of a Keplerian ellipse, rather than requiring the equivalent of a strict circular orbit. Maxwell does not discuss his reasoning. It can be justified, however, by

[†]Or, strictly speaking, an inflection point; but it's hard to see how this could actually happen for any reasonable hoop.

[‡]Indeed, it raises some question about the general applicability of Maxwell's treatment. Dealing with such a question in detail, however, would take us rather far from Herschel and Laplace. In any case, Maxwell's further sections on the stability of other kinds of ring seems to take care of any doubts we might have on the subject.

appealing to the structural strength of the hoop: if the motion is not uniform, the centrifugal force cannot be expected to balance gravitational force exactly everywhere, and stresses would be set up in the hoop that no material could support.

Maxwell then proceeds to allow small variations in the angular quantities and distance, puts them in exponential form, and investigates the conditions under which the exponents remain imaginary or negative: a perturbation analysis of the familiar kind. Expanding the mass distribution around the ring in a Fourier series, he finds that only the first few coefficients have any effect on the (infinitesimal) stability, allowing a useful level of simplification.

The salient result, for Maxwell, was that for a loaded ring, an object equivalent to ‘a single heavy particle placed at a point on the circumference of the ring’ (p. 310) at this level of Fourier approximation, the ring must be off centre by an amount between 0.81565 and 0.8279 of the radius in order to be stable. This not only requires a very ‘finely-tuned’ system (to use a current term), it is quite ruled out by observation.

Maxwell then proceeds to investigate the other possibilities for the nature of Saturn’s rings, eventually concluding that they could only be made up of innumerable tiny objects, each in a separate orbit around the planet. This is the result most remembered from his analysis and the one accepted today.

Another look at stability

Having outlined the results of Laplace and Maxwell on the subject of Saturn’s rings considered as rigid hoops, it is instructive to look at another approach. This uses energy and angular-momentum arguments, a more modern way of working, though probably understandable by these consummate mathematicians.

The situation is diagrammed in Fig. 2. The planet’s centre of mass is at S , that of the ring at R , and these rotate around the barycentre B as measured by the angle θ between the line of centres and a fixed direction (not drawn, for clarity). The ring rotates about its centre of mass as measured by an angle ϕ between the line CR and a fixed direction (also not drawn) which may be different from θ (in particular, it may change at a different rate). With the mass of the planet M and of the ring m , we may write down the kinetic energies of angular motion for the planet about the barycentre, the ring about the barycentre and the ring about its centre of mass respectively as

$$\begin{aligned} T_M &= \frac{1}{2}M(SB)^2\dot{\theta}^2 \\ T_R &= \frac{1}{2}m(BR)^2\dot{\theta}^2 \\ T_r &= \frac{1}{2}m(r^2 - b^2)\dot{\phi}^2 \end{aligned} \tag{1}$$

The kinetic energy of linear motion is

$$T_L = \frac{1}{2}M(\dot{SB})^2 + \frac{1}{2}m(\dot{BR})^2 \tag{2}$$

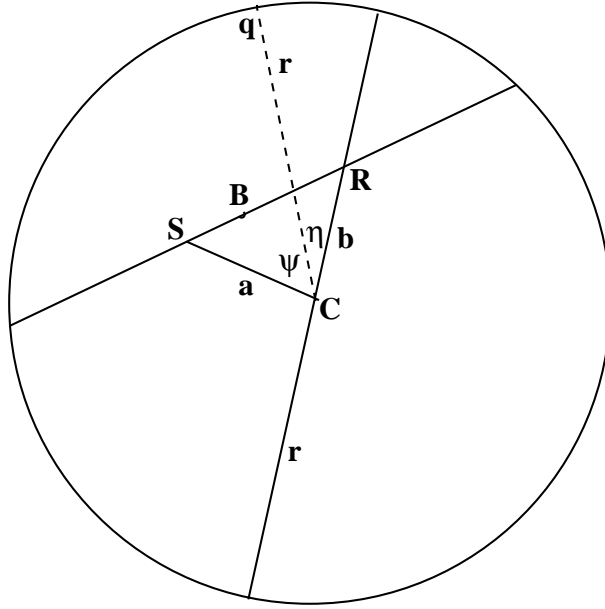


Figure 2: Figuring the dynamic stability of a solid ring about a planet. The ring is drawn, with geometric centre at C and radius r ; if its mass distribution is not uniform the centre of mass is offset by a distance b and is located at R . Its rotation about its centre of mass is measured by the angle ϕ (not shown) from a fixed reference direction. The centre of mass of the planet, idealised as a sphere, is at S , offset from the centre of the ring by the distance a and from the barycentre of the whole system by the distance SB . The line joining the centres of mass, SR , rotates as measured by the angle θ (also not shown). A typical point on the ring is represented by q .

The total angular momentum, which is constant, is

$$J = M(SB)^2\dot{\theta} + m(BR)^2\dot{\theta} + m(r^2 - b^2)\dot{\phi} \quad (3)$$

The distribution of mass about the ring is given by a function $\nu(\eta)$, where η is the angle measured at C from the line CR . It is related to the eccentricity of the centre of mass of the ring by

$$b = \int_0^{2\pi} \nu r \cos \eta d\eta \quad (4)$$

and, of course, is positive (or zero). We will also need the angle ψ , measured at C from the line SC .

The gravitational potential energy of the ring with respect to the planet is

$$V = -GMm \int_0^{2\pi} \frac{\nu d\psi}{\sqrt{a^2 + r^2 - 2ra \cos \psi}} \quad (5)$$

(note that, since both ψ and η are measured from C and in each equation given go around the full circle, we may freely convert between them). To work out the dynamics we shall have to shift from a to SR as the variable of interest; although a is more directly observable, SR allows a more direct connection to the expressions of kinetic energy. While a and b are small all three quantities are of the same order, so in examining the situation close to symmetry it suffices to work out the behaviour of either a or SR . The conversion between them is given by

$$a^2 = b^2 + (SR)^2 - 2bSR \cos(\phi - \theta). \quad (6)$$

To clean things up a bit, we introduce the parameters $\mu = m/M$, $\sigma = SR/r$, $\alpha = a/r$ and $\beta = b/r$.

The total kinetic energy is

$$T = \frac{M}{2r^2} \frac{\mu}{\mu+1} \dot{\sigma}^2 + \frac{M}{2r^2} \frac{\mu}{\mu+1} \sigma^2 \dot{\theta}^2 + \frac{M}{2r^2} \mu (1 - \beta^2) \dot{\phi}^2. \quad (7)$$

We now introduce the parameter $\gamma = \dot{\phi}/\dot{\theta}$, the rate at which the ring rotates about its centre of mass compared to the rate of rotation of the centres of mass about the barycentre; for a symmetrical ring it will be unity. We can now write the total angular momentum as

$$J = \frac{M}{r^2} \left(\frac{\mu}{\mu+1} \sigma^2 + \mu(1 - \beta^2)\gamma \right) \dot{\theta} \quad (8)$$

and use this expression to substitute for $\dot{\theta}$ in the kinetic energy, giving

$$T = \frac{\left(\frac{M}{2r^2} \frac{\mu}{\mu+1} \sigma^2 + \frac{M\mu}{2r^2} (1 - \beta^2) \gamma^2 \right) J^2}{\left(\frac{M}{r^2} \frac{\mu}{\mu+1} \sigma^2 + \frac{M\mu}{r^2} (1 - \beta^2) \gamma \right)^2} + \frac{M}{2r^2} \frac{\mu}{\mu+1} \dot{\sigma}^2. \quad (9)$$

There is some obvious cleaning up to do in this expression. In addition, we will want to compare the total angular momentum of the eccentric ring to the situation in which all the angular momentum is contained in the ring material performing a circular orbit about the planet; in the latter case the angular momentum is

$$J_0^2 = GMm^2r \quad (10)$$

and we will introduce the angular momentum parameter $j = J/J_0$.

We are now in a position to write down a useful expression for total energy of the system, arriving at

$$\begin{aligned} E &= \frac{1}{2}Mr^2 \frac{\mu}{\mu+1} \dot{\sigma}^2 \\ &+ \frac{GMm}{r} \frac{j^2 \left(\frac{\sigma^2}{\mu+1} + (1-\beta^2)\gamma^2 \right)}{\left(\frac{\sigma^2}{\mu+1} + (1-\beta^2)\gamma \right)^2} \\ &- \frac{GMm}{r} \int_0^{2\pi} \frac{\nu d\psi}{\sqrt{1 + \alpha^2 - 2\alpha \cos(\phi - \theta)}}. \end{aligned} \quad (11)$$

This may be rearranged to form

$$\begin{aligned} \frac{1}{2}Mr^2 \frac{\mu}{\mu+1} \dot{\sigma}^2 &= E - \frac{GMm}{r} \times \\ &\left\{ \frac{j^2 \left(\frac{\sigma^2}{\mu+1} + (1-\beta^2)\gamma^2 \right)}{\left(\frac{\sigma^2}{\mu+1} + (1-\beta^2)\gamma \right)^2} - \int_0^{2\pi} \frac{\nu d\psi}{\sqrt{1 + \alpha^2 - 2\alpha \cos(\phi - \theta)}} \right\} \end{aligned} \quad (12)$$

the equation expressing the dynamics of a particle of mass $Mr^2\mu/(\mu+1)$, (one-dimensional) coordinate σ and (constant) total energy E in an effective potential given by the expression involving the brackets. The radical in the denominator of the integral, in terms of useful quantities, is

$$1 + \beta^2 + \sigma^2 - 2\beta\sigma \cos(\phi - \theta) - 2\cos\psi \sqrt{\beta^2 + \sigma^2 - 2\beta\sigma \cos(\phi - \theta)}. \quad (13)$$

For the ring to have stable motion, the effective potential as a function of σ must have a local minimum.

Let us consider first the completely symmetrical situation. In that case $\beta = 0$, $j = \gamma = 1$, $\sigma = \alpha$, $\phi = \theta$ and $\nu = 1/2\pi$, and Eq. 12 gives

$$V_{\text{eff}} \propto \frac{\mu+1}{\sigma^2 + \mu+1} - \frac{1}{2\pi} \int_0^{2\pi} \frac{d\psi}{\sqrt{1 + \sigma^2 - 2\sigma \cos\psi}}. \quad (14)$$

The left-hand term, the one introduced by the rotation of the ring, is a monotonically decreasing function of σ , and not a very strongly varying function at that. The integral is a monotonically increasing function of σ , with increasing derivative, becoming singular at $\sigma = 1$; thus the whole expression is monotonically

decreasing. There is no local minimum and motion is unstable. The addition of rotation does not stabilise the ring, though it does modify the dynamics somewhat. Herschel’s second assertion about Saturn’s rings is correct.

Consider, now, Eq. 12 in the case of a ring close to symmetry. Since β is small and always occurs added to or subtracted from quantities that are not, it will have no qualitative effect on V_{eff} . Similarly, j and γ will be close to unity, the first only rescaling the angular-momentum term slightly and the second (since it shows up to the same degree in numerator and denominator) having no strong effect. A small eccentricity due to asymmetric mass does not stabilise the ring. Herschel’s third assertion concerning Saturn’s rings is incorrect.

What, then, of Herschel’s intuition that a loaded ring would act like a ‘sluggish’ satellite? If we take the limiting case in which all the mass of the ring is concentrated at a single point, we obtain

$$V_{\text{eff}} = \frac{GMm}{r} \left\{ \frac{j^2(\mu + 1)}{\sigma^2} - \frac{1}{\sigma} \right\} \quad (15)$$

which indeed has a minimum[§]. A ‘ring’ with the proper combination of total energy and angular momentum would be confined within certain values of σ ; alternatively, if a circular orbit at the minimum value of σ is perturbed, the ring stays within a small distance of this value. (These are *different* definitions of stability, as will be discussed below.)

Somewhere, then, in parameter space between a symmetric ring and a totally asymmetric one there is a border between stability and instability. Seeking to define the border in a multidimensional parameter space would be tedious (it depends on the distribution of mass in the ring, which can in principle be very complicated). For our purposes it is sufficient to note that it is not near the symmetric case, but that Herschel’s intuition is qualitatively correct.

Cutting-edge observations, dynamic stability and philosophy

Although all the details of this episode concern questions, techniques and equipment that have long been left behind by astronomy, there are a number of lessons that are every bit as applicable for us now as they were for Herschel and his contemporaries. I will divide them up into three areas: the treatment of observations at the forefront of research; the formulation and interpretation of mathematical analysis; and some aspects of the practical philosophy of science.

Observationally, we have a situation in which one measurement with cutting-edge instrumentation (placing all of Struve’s numbers together as one measurement with a given probable error) gives a formally significant result. It is not confirmed by other efforts made with admittedly less capable equipment. Certainly the same kind of situation has arisen since, and no doubt will arise again. One’s reaction to such a singular observation will generally depend on one’s

[§]For a ring/satellite of negligible mass compared to the planet ($\mu = 0$) and having the same angular momentum as a symmetrical ring centrally placed ($j = 1$) the minimum is at $\sigma = 2$, placing the planet *on* the (now merely conceptual) ring. This suggests that any stable configuration would also be rather eccentric, in agreement with Maxwell’s result.

prejudices, perhaps dressed up with Bayesian numbers; but in any case a single result from a single source is difficult to handle.

In this general situation I suggest that a useful attitude is that of Sir A. S. Eddington in a different context (introducing the first measurements of the gravitational redshift of the White Dwarf companion of Sirius¹⁰):

I have said that the observation was exceedingly difficult. However experienced the observer, I do not think we ought to put implicit trust in a result which strains his skill to the utmost until it has been verified by others working independently. Therefore you should for the present make the usual reservations in accepting these conclusions.

For the next part of this section we turn from observation to theory, in particular the construction and interpretation of calculations.

The mathematics laid out by Laplace, explained by Bowditch and referred to by Herschel contain no errors. That is, the manipulation of formulae *once the problem had been set up* was correct. But it is quite possible to do the mathematical manipulations correctly and still come up with the wrong answer. In this episode we have examples of three ways of doing that: first, by ignoring something that could be important (the motion of revolution of the ring); second, by making a mistake in formulating the physical situation (calculating static stability for an object not in equilibrium).

The third way is more subtle, lying in the transition from words to mathematics: what do we mean by ‘stable?’ Here, there appears to have been a confusion of static with dynamic stability, or perhaps a lack of recognition that there is a difference. Herschel’s use of the term *unstable equilibrium* and Laplace’s treatment of the symmetrical ring are unexceptionable—unless things are in motion. An object in what we might call a stable orbit around a planet, for instance, is *not* in equilibrium; there are unbalanced forces on it and it is accelerating all the time; so one must come up with a new definition in order to deal with dynamic stability.

As noted above, Maxwell made the distinction and formulated a precise definition of dynamic stability: a system is stable if a small disturbance gives a small periodic deviation from the original motion. There are other possible definitions, for example this from the textbook of Thomson and Tait¹¹:

The actual motion of a system, from any particular configuration, is said to be *stable* if every possible infinitely small conservative disturbance of its motion through that configuration may be compounded of conservative disturbances, any one of which would give rise to an alteration of motion which would bring the system again to some configuration belonging to the undisturbed path, in a finite time, and without more than an infinitely small digression. If this condition is not fulfilled, the motion is said to be *unstable*.

This is a rather more restrictive definition than Maxwell’s: it requires the disturbances to be conservative and a return, somewhere, to the original path after

a finite time. One could imagine situations satisfying Maxwell's definition and not Thomson and Tait's. Whittaker¹² gives a choice of definitions, one concerning deviations from a particular kind of motion and attributed to Klein and Sommerfeld:

... *steady motion* ... is defined to be a motion in which the non-ignorable coordinates of the system have constant values, while the velocities corresponding to the ignorable coordinates have also constant values.

...

The steady motion is said to be *stable* if the vibratory motion tends to a certain limiting form, namely the steady motion, when the initial disturbance from steady motion tends to zero. (p. 193, §83)

Alternatively,

The word *stability* is often applied to characterise types of motion in which the moving particle is confined to certain limited regions ... (p. 417, §184)

a definition which is noted as having been used by Hill, Bohlin and Darwin. It is quite a different definition, mathematically, from the previous ones, in that it allows deviations from a given path that are not small. Clearly there are situations satisfying this criterion and not the others.

The general lesson to take away from this dictionary exercise is that the translation from language ('is the ring stable?') to mathematics may not be straightforward or single-valued, and unexpected subtleties may appear. This is the time to be especially clear and careful, or you may wind up with mathematically elegant and sophisticated calculations that don't, in fact, prove anything.

Finally, we move into the practical philosophy of science; that is, how scientists (especially) expect the process of discovery to be structured. While its specific applicability to the episode of Saturn's rings is speculative, I think the following passage from Herschel's later book² is relevant:

Almost all the greatest discoveries in astronomy have resulted from the consideration of what we have elsewhere termed RESIDUAL PHENOMENA, of a quantitative or numerical kind, that is to say, of such portions of the numerical or quantitative results of observations as remain outstanding and unaccounted for after subducting and allowing for all that would result from the strict application of known principles. (p. 769, §856)

Herschel thus expects to look for new discoveries in the form of small quantities not previously discernable, perhaps by using a newer, bigger telescope. This is a reasonable expectation in any mature science (the big, obvious things have already been accounted for) and arguably retains its utility as a general rule. The problem is, of course, that when one hunts for signals down among the noise, one can mistake noise for signal.

A different point of practical scientific philosophy comes up in the fact that, when two independent results agree, there is a very strong inclination to conclude that they must both be true. As expressed by Johannes Kepler¹³, ‘how easily the false disagrees with itself, and on the other hand how reliably truth is consistent with truth’ (Ch. I). But sometimes the false agrees with itself. *Sometimes errors reinforce each other*, as well as true results. Kepler¹³, again, was familiar with this:

Indeed it is not the least important part of being shrewd to beware of accidental associations of this kind, which, as the Sicilian siren once detained seafarers with her singing, detain those engaged in philosophy by the pleasure of their apparent beauty and their neatness of fit ... so that they cannot attain the predetermined goal of knowledge. (Ch. XIV)

The phenomenon of reinforcing errors is the most important lesson of this episode, the one to retain when all the details are gone. I suspect most scientists do not bear it sufficiently in mind, and most non-scientists aren’t aware of it at all. But it can confidently be expected to appear whenever one is searching for patterns amidst the noise on the edge of current research.

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